The thesis of this paper is that some of the current problems of mental illness may be illuminated by historical analysis. General paresis, endemic cretinism, and the psychosis of pellagra are examined in this context, and several conclusions emerge that have implications for further research on mental illness.

PATTERNS OF DISCOVERY AND CONTROL IN MENTAL ILLNESS

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AS POSSIBLE FACTORS in the etiology of general paresis, Krafft-Ebing in 1877, listed such diverse elements as heredity, the menopause, trauma to the head, excessive heat and cold, fright, alcoholism, excessive venery, exhaustion as a result of earning a living, and the smoking of from 10 to 20 Virginia cigars daily.[1] While sexual dissipation is included, there is no specific mention of syphilis. Indeed as late as 1898, the pathologist, Virchow, vigorously denied the syphilitic origin of general paresis.[2] Yet, 20 years before Krafft-Ebing's discussion, in 1857, Esmarch and Jessen had already postulated a causal relationship between syphilis and mental disorder,[3] and 15 years after Virchow's dogmatic denial, Noguchi and Moore demonstrated Treponema pallidum in the brain of paretic patients.

Throughout most of this period, however, despite accumulating evidence in favor of an infectious origin, causal explanations of general paresis in psychologic and social terms continued to enjoy credit. Indeed, the situation was not unlike that which characterizes current efforts to unravel and to achieve an understanding of the etiology of the functional psychoses. No single view on the causation and pathogenesis of these disorders is currently regarded as generally acceptable. Rather a multiplicity of diverse viewpoints prevails.[5] Schizophrenia may be taken as an example. Bowman and Rose point out that "A great many explanations of what goes on in schizophrenia have been proposed and attempts made to account for the clinical data by relating them to one theoretical framework or another. There are explanations which are largely psychodynamic, cultural or sociological, psychosomatic, biological (including hereditary), and biochemical."[6]

This situation has in part created and in part been affected by a concept of etiology which is as inchoate as it is all-embracing. The most succinct expression of this standpoint was offered in 1955 by the National Advisory Mental Health Council. "The concept of etiology as embraced by modern psychiatry," it said, "differs from the simple cause and effect system of traditional medicine. It

subscribes to a 'field theory' hypothesis in which the interactions and transactions of multiple factors eventuate in degrees of health or sickness."[7] From this idea it is not far to the recent statement by Menninger and his co-workers that "There are no natural mental disease entities," or to the logical corollary of this retreat into resignation—a unitary concept of mental illness and health.[8]

In the face of the confusion and obscurity, not to say obscurantism, which mark numerous current endeavors to understand and to control mental illness, a retrospective analysis of the ways in which certain earlier problems of mental disease have been untangled and controlled may perhaps shed light on the nature of current difficulties. Do these earlier instances exhibit some pattern of development, or of discovery? Can certain types of data emerge, or be understood only at certain levels in such a pattern? Are we at present, perhaps, expecting too much from too little? Is the concept of etiology in terms of "anything goes" hindering the most effective ordering of the available data? Are there cultural biases that predispose clinicians and research investigators to lend credence more readily to some reports than to others, or to seek in one direction rather than in another? With such questions in mind, the history of general paresis, cretinism, and the psychosis of pellagra will now be examined.

General Paresis

Appearing in Europe toward the end of the fifteenth century as an acute epidemic disease, syphilis spread rapidly through the population. Following the outbreak of the disease, however, there was no definite mention for almost two hundred years of any clinical manifestations that can with certainty be attributed to general paresis.[9] Indeed, it was not until the early decades of the nineteenth century that general paresis was identified as a separate disease entity. How can we explain this striking fact? Were there no cases of paralysis? Did the disease appear but was not recognized? While it is doubtful whether these questions can ever be answered with any finality, a plausible explanation may be advanced.

Account must be taken first of changes in concepts of disease. Ancient and medieval physicians did not generally distinguish different specific diseases, but were concerned rather with various groups of symptoms exhibited by sick people. A new and important approach to the problem of the nature of disease was made, however, in the seventeenth century by Thomas Sydenham. Not primarily concerned with the ultimate nature of disease, he directed his attention to the phenomena observed at the bedside. Furthermore, he thought in terms of diseases rather than of disease as a general condition, and was firmly convinced that it was possible to draw up a complete picture of each disease, much as one described a plant or animal. By abstracting the signs and symptoms which he saw repeatedly in sick people, and arranging them in a coordinated manner, Sydenham arrived at a concept of a disease as an entity, an objective thing in itself.

This idea of disease description and differentiation was taken up and developed during the eighteenth century. However, these efforts to describe and classify morbid conditions had great shortcomings. Symptoms furnished the only basis upon which diseases could be distinguished, with the result that symptoms exhibiting a superficial similarity, but differing widely in pathogenesis and significance were often grouped together. There was as yet no clear-cut idea of the intimate relationship between organ lesion and clinical observation. This situation is reflected in the earliest reports relating to general paralysis.

Beginning with Willis in 1672, a number of writers — Haslam (1798), Cox (1804), Esquirol (1805, 1814), Georget (1820), Delaye (1824), Calmeil (1826) —described a kind of paralysis associated with mental illness.[10] The clinical features of the condition were described quite accurately by these authors, but they were not aware that they were dealing with a separate and distinct disease entity. Some considered the paralysis an illness causally independent of the associated dementia, or as a complication of various forms of mental disorder. Clinical observation was not enough to establish general paresis as a disease entity, particularly in view of its chronic development, its variegated manifestations, and unknown etiology.

Furthermore, a number of other factors must be taken into account. In the first place, syphilis was originally an acute disease which killed a number of its victims before tertiary manifestations could develop or become apparent. Second, the mortality in earlier periods was so high because of epidemics, famines, wars, and diseases due to insanitary conditions that many possible candidates for tertiary lues died much earlier in life. In seventeenth century France, for example, of 1,000 live births, only 475 individuals attained the age of 20, 318 the age of 40, and 130 the age of 60.[11] Third, not all syphilitics develop general paresis. Obviously, in order to arrive at the concept of a distinct disease entity, something more than mere clinical observation was necessary.

The clinical approach to the study of disease was paralleled by an anatomical one. Anatomical investigation had been sedulously cultivated for centuries, and in the course of innumerable dissections and autopsies, a mass of pathological observations was collected. Gradually the view gained ground that the reactions observed in human beings under the stress of disease are related to the organ lesions found after death. This idea was first given effective expression by Morgagni in his work "De sedibus et causis morborum" which appeared in 1761 at Venice. In this famous book, Morgagni showed conclusively that disease has a definite seat in the organs and that pathological changes in organs are responsible for most symptoms. By firmly linking the symptoms which constitute the clinical picture of a disease to an anatomical base which explained them, Morgagni opened up the possibility of observing the mechanism of disease and indicated the path to be followed by future research. The fusion of the clinical and anatomical approaches and their systematic application was the great contribution of the Paris school of clinical pathologists from 1800 to 1850. As a result of their labors there emerged from the earlier uncertainties and confusion relatively clear and critical pictures of diseases based upon the idea that there was a definite connection between the clinical findings and the lesions observed in various organs at autopsy.

Application of this method to the problem of dementia and paralysis led eventually by the middle of the nineteenth century to the delineation of a disease characterized by disturbances of motility, mental derangement, and pathological changes in the nervous system underlying these phenomena. This contribution was essentially the work of the French school. In 1822, Bayle offered a concept of general paralysis as a disease entity marked by several distinct stages. The patients exhibited at first a variety of grandiose ideas and excitement; then further disturbances of intellectual function, agitation, and paresis; and finally a greater or lesser obliteration of ideas, extensive paralysis, and sometimes paroxysms of agitation. Bayle felt that the symptoms did not occur as part of any other mental illness.[12]

This view was not immediately ac-

cepted. After all there were cases of progressive general paralysis without mental symptoms. Most of the early studies dealt with asylum populations, and it was felt that the findings did not apply to other groups. Furthermore, there was a considerable lack of clarity as to the sequence and relationships of the mental and physical aspects of the condition. Today it is clear that the widely divergent views were based on the study of different facts.

Some considered the disease as a complication and consequence of various disorders, for example, Esquirol (1838). Others regarded it as a progressive disease with or without mental derangement. Baillarger, in 1847, stressed the division of general paralysis into two categories with or without mental symptoms, and indicated the great importance of differential diagnosis. Since he considered the disturbance of motility primary, Baillarger attempted in the same year to test patients by means of a galvanic current.

By the fifties and sixties, however, the clinical and pathological alterations had been quite accurately delineated. Falret, in 1853, established the nature of the disease in terms, which, broadly speaking, are still valid. He described it as a specific mental illness with characteristic paralyses, a typical course and typical psychotic manifestations.[13] Meanwhile other investigators had been making studies of the pathological findings in the nervous system. Parchappe in 1838 reported inflammatory softening of the cerebral cortex in cases of general paralysis, and by 1858 he had autopsied 322 cases confirming his earlier findings.

With the general outlines of the condition fairly well established, attention shifted to the differentiation of the various forms of paralysis and to the question of etiology. At the same time, German contributions to these problems became increasingly prominent and significant. Hoffmann reported in 1848 that one-sixth of the patients at Leubus were paralytics, and in 1851 Stolz published similar figures for the asylum at Hall in the Tyrol.[14] The first important German-language clinical description of general paresis, a paper by Duchek of Prague, in which he depicted the four phases of the disease, also appeared in 1851. Attention was turned to the differentiation of general paresis from tabes dorsalis, and in 1871 a means was found to do this. Westphal observed that the patellar tendon reflex was absent in tabes dorsalis, and when it was shown that this reflex was present in general paresis it became possible to separate cases of the two diseases. By the eighties, the clinical phenomena had been quite well defined, and to them were added the results of histopathological studies. While opinions as to the nature of the histopathological changes remained divided for a number of decades, they did establish the physical causation of general paresis. These studies found their culmination in Alzheimer's description in 1904 of the microscopic changes in the brain of general paretics.[15]

The histopathological studies not only provided more precise knowledge of the pathogenesis of general paralysis, but they reflect another shift in the study of the disease, a shift to the laboratory. And it was ultimately through the laboratory that the etiology of general paralysis was clarified and established. While the early writers on this subject considered the question of etiology, their views were quite diffuse. Bayle discussed causation in two broad categories, moral and physical. Social and psychological factors which comprised the former group were considered as potent etiologically as the physical causes. The more frequent occurrence of the disease among former officers and soldiers of the Napoleonic armies was explained in terms of the privations experienced by these men, the terrors of war, their excessive drinking, and the disappoint-

ment resulting from the defeat of Napoleon. Emotional causes such as violent love or profound jealousy seemed to be involved in some cases. In others, excessive intellectual activity, grief, sorrow, and similar factors predisposed to the disease. Syphilis was considered a possible etiological factor, but only one of several. Hereditary tendencies seemed to be more important in some instances, and the preponderance of the male sex among the patients was considered significant. The role assigned to syphilis is characteristically summed up by Bayle. "About one-fifth of the patients whom I observed," he wrote, "indulged in venereal excesses and often contracted syphilitic ailments. However, excesses of this kind and the illness which follows them are so frequent that I would not venture to include them among the predisposing causes of chronic meningitis. In addition, a physician, M. Cullerier, whose opinion has great weight in this matter, thinks that syphilis has no influence whatever upon the development of mental alienation."[16]

Throughout the first half of the nineteenth century and well into its second half, French clinicians and those who took their cues from them, tended to stress sociopsychological and hereditary factors in the etiology of general paralysis. For example, Esquirol mentions that one-twentieth of those admitted to the Salpêtrière were formerly prostitutes. After indulging in all kinds of excesses, they succumb to a form of dementia complicated by paralysis. Yet he did not associate the occupational hazard of these women—syphilitic infection—with their mental illness. Other examples might be cited. Indeed the situation is all the more striking because it was during this period that the French school gained one of its greatest distinctions by reestablishing clarity in the understanding of clinical syphilis. John Hunter had thrown the subject into confusion by denying the duality of syphilis and gonorrhea. Beginning with Ricord in 1837, the French school developed the clinical knowledge of syphilis as far as could be done before the discovery of the organism in the twentieth century. Why then was syphilis not more closely associated with general paralysis in terms of etiology?

An answer to this question must take account of the climate of opinion in which these studies were carried on, and of the elements in it by which they were affected. Since the eighteenth century the view was widely held that civilization was an important factor in the causation of mental illness.[17] Furthermore, during the nineteenth century many psychiatrists believed that insanity must be increasing because society was becoming more complex. Edward Jarvis, an American physician, concluded in 1851 that insanity is "a part of the price we pay for civilization. The causes of the one increase with the developments and results of the other."[18] This belief in the rising tide of madness is a theme that is played with numerous variations throughout the century.

Ackerknecht has made the provocative suggestion that the belief in a progressive increase of insanity during the nineteenth century is an aspect of the belief in progress, that the belief was firmly held even when there was no firm basis in fact because the greater prevalence of mental illness was evidence of more advanced civilization, since civilization was considered a basic element in its causation. As Jarvis put it, insanity was the price paid for the high level of civilization attained by nineteenth century western Europe as a consequence of the Industrial Revolution. Not all investigators subscribed to this view. Esquirol, for example, believed the rise in mental illness was more apparent than real. Nevertheless, the very fact that the problem received so much attention and was so hotly debated meant that special areas of in-

terest like general paralysis were more likely to be affected. Thus, it is not surprising to find the question raised: Is general paralysis a modern disease, and is it increasing?

Lunier, the first historian of the disease, commented in 1849 that while paresis had probably occurred in earlier periods, there was no doubt of its increase with the advance of civilization.[19] Moreau, in 1850, saw the progress of civilization as the cause of the increasing frequency of general paralysis, and Baillarger begins his account of the early history of the disease with the statement: "The occurrence of this terrible disease seems to increase daily."[20] Even at the end of the nineteenth century and in the early years of the twentieth century, at a time when the role of syphilitic infection in the etiology of general paresis was becoming increasingly clear, attention was still focused on the rise in the number of cases and the possible social causes of this development. Krafft-Ebing, in 1895, attributed this increase to changes in social conditions which had brought about a physical, and even more specifically neurological deterioration of large segments of the population.[21] Kraepelin,[22] Rüdin,[23] and others[24] expressed similar views more than a decade later. According to Kraepelin the civilized peoples had lost certain protective mechanisms, which are still present among primitives and which make it difficult among them for paralysis to develop from lues.

This approach was further reinforced by the introduction of Morel's degeneration hypothesis in 1857.[25] He defined degenerations as pathological deviations from the normal type, which are transmissible through heredity and which develop progressively to death. Degeneration was due to intoxications, social milieu, pathological temperament, heredity, and acquired or congenital insults of various kinds. Once acquired, the various generations of a family went inexorably to their doom. Mental illness was the degenerative condition par excellence, and for many general paralysis became a degenerative disease. This theory of predestination in terms of an original biological sin exerted a powerful attraction on many psychiatrists, and directed their attention away from any concept of specific etiology. This was a comfortable position to occupy at a time when specific etiology in general was largely out of favor, and epidemiological theories were framed in vague environmental terms.[26]

Under such circumstances, it becomes clearer why syphilis seemed to be no more significant in the etiology of general paralysis than other factors. Nevertheless, it was in the very year in which Morel brought out his degeneration hypothesis that Esmarch and Jessen proposed a specific etiology for the disease with syphilis as the essential cause.[27] Interestingly enough, Griesinger, who insisted that mental diseases are illnesses of the brain, rejected this hypothesis as improbable. Nonetheless, the evidence in support of this assumption accumulated slowly but surely. In a statistical study, Jaspersen, a Dane, showed in 1874 that 90 per cent of all paralytics had previously had syphilis.[28] By 1894, Fournier was able to cite a number of statistical studies which showed that syphilitic infection was far more common in the history of general paretics than in the past of other mental patients. It was found in 65 per cent of the former and only in 10 per cent of the latter. "Such being the case," Fournier concluded, "how in any logical sense is it possible for syphilis to have no connection with the causation of general paralysis?"[29]

The answer of course became increasingly evident. From a variety of sources evidence accumulated and made it more and more difficult to dispute that the close correlation between syphilis and general paresis was an etiological one.

A striking piece of evidence was provided by Krafft-Ebing in 1897. Nine paretics with no history of syphilitic infection were inoculated with luetic material. None of the experimental subjects developed secondary symptoms, and the inference was drawn that they had previously been infected.[30] This observation is obviously similar to those upon which Colles based his dictum of 1837 that a woman who gave birth to a syphilitic child was herself immune to syphilis.

Further important support came from the laboratory. Under the influence of Virchow's cellular pathology, a number of investigators undertook to study the pathological changes in tissues and cells. With the creation of the conceptual and technical bases of microbiology, others began to study the immunological properties of blood and other body fluids. In 1890, Quincke showed that cerebrospinal fluid could be obtained by direct lumbar puncture, and investigations of this material were soon turned to diagnostic purposes. Study of the chemical and cytological components of the cerebrospinal fluid revealed a combination of findings characteristic of general paresis. In 1906, Wassermann and his coworkers evolved the complement-fixation test known by his name, and other tests were developed later. The serological tests not only confirmed the syphilitic nature of active lesions, but also showed that a latent syphilis could be present in an individual, even though there were no active lesions. Lange introduced the colloidal gold test in 1912.

By this time, the syphilitic etiology of general paresis was quite generally accepted. As Mönkemöller put it in 1911, "the opponents of this causal connection are almost completely silenced."[31] Finally, in 1913, all remaining doubts were set aside by the demonstration of the syphilitic organism in paretic brains. More than two hundred years after the problem of general paresis first appeared dimly on the medical horizon, this discovery finally brought clarity into a perplexing etiological problem and made possible an approach to rational methods of control. Obviously, if syphilis was the ultimate cause, prevention of paresis meant prevention of syphilis. Where this could not be achieved, early detection and treatment of syphilitic infection were in order. And it was now possible to endeavor to develop a rational therapy directed at the cause. As is well known, the steady decline in the incidence of general paresis has been due to the measures of control instituted in the last forty years on the basis I have just described.

I have devoted a large part of this discussion to general paresis because it illustrates clearly a number of important points. One is the problem of conceptualization. Whether one or more clinical observations represent a single disease entity or a group of phenomenologically similar, but basically unrelated disorders is a matter of fundamental importance. In order to establish etiology, whether single or multiple, one has to have some reasonably clear notion of the entity to which etiological factors are related. This problem is not unique to mental illness but applies as well to other diseases. For a long time this was a problem in pulmonary tuberculosis, where a number of conditions with common clinical symptoms were subsumed under the common label "consumption." Eventually, consumption was separated into several distinct entities with differing etiologies. Very probably the understanding of schizophrenia is at a similar level today.

In short, conceptualization at a clinical level is the first stage in understanding a disease. Where a disease occurs in acute form and with striking manifestations, the observations upon which clinical conceptualization is based enable the structuring which results in the discovery of a disease entity. However,

where diseases are of long duration and the clinical observations cannot be as sharply delineated or controlled, clinical conceptualization is much more difficult. As a result, the search for etiologic factors may very possibly lead into blind alleys. The search for meaningful associations of pathogenetic factors must take off from a clinical base, but the clinical data must also be related to observations and data derived from other levels of investigation. This is the second stage in the pattern of discovery. But if the clinical data or variables are not well defined, the significance of information derived from other levels, for example, the chemical or the psychological, may be highly dubious. Horwitt has clearly shown how much research in schizophrenia is vitiated by environmental artifacts based on inadequate recognition or delineation of the involved variables.[32] This does not mean that one should wait until the mental disorders are classified and recognized in pure phenomenological form before beginning the quest for causation, but it does emphasize the need for critical evaluation and understanding of the level at which the problem is being studied.

Levels of investigation may also be considered as frames of reference within which investigators carry on their work. As a reflection of the level of knowledge, the frame of reference tends to focus the investigator's perception of the problem. Where the level of knowledge is essentially clinical and observational, with few correlates on other levels, there is wide latitude for the postulation of putative etiological factors and processes. A recent article by Kubie on the possibility of a preventive psychiatry is pertinent here.[33] By postulating an almost ubiquitous neurotic process very early in life, and by relating it directly to some of the most complex social institutions, Kubie predetermines his basically negative answer to the possibility of a preventive psychiatry. Furthermore, by operating within a psychoanalytic frame of reference, and by insisting on the independence of the psychological variables, the postulated neurotic process becomes part of a closed system. In these terms, it can neither be accepted nor rejected out of hand. Ultimate acceptance or rejection would depend on the extent to which data can be obtained outside this framework to support the postulated etiologic factor or process. Here too the case of general paresis is instructive. Data to support the syphilitic etiology came not alone from the clinical association of syphilis and general paralysis, but in addition from pathological studies, statistics, microbiology, and immunology. Within this constellation the social and psychological elements then found their appropriate places. In a sense, one can think of linked open conceptual systems where what happens in one system can have an effect on the others.

Pellagra and Cretinism

The latter point may be usefully examined in the histories of pellagra and cretinism. Pellagra was first observed and described in Spain by Gaspar Casal, a physician of Oviedo.[34] He saw the first cases of the disease, which he called mal de la rosa, in 1735, but his findings were not published until 1762, three years after his death.[35] Thirty-six years after Casal saw the first cases, Francesco Frapolli reported the existence of the disease in Lombardy, and named it pellagra (rough skin), the term by which it is still designated.[36] Thereafter pellagra was observed in France, Rumania, the United States, and in a number of other countries. There is no need here to delve deeply into the history of pellagra. Those who are interested may consult the works of Roussel (1866) and Hirsch (1883-1886).[37]

Significant for this discussion, how-

ever, is that pellagra appeared in comparatively recent times as a disease hitherto unknown. The clinical picture was described quite accurately from the very outset — the skin manifestations which gave the disease its name, the general weakness and the systemic complaints, and finally the psychotic manifestations associated with the disease. Clinically, the disease was conceived initially as a distinct entity. Also practically from the outset it was observed that pellagra occurred almost exclusively among the poorest of the rural population. Furthermore, in the search for the etiology of pellagra attention turned very early to the use of maize as food. While some attempts were made to explain the etiology of pellagra in terms of contagion or heredity, such explanations were not taken very seriously except for a short period early in the twentieth century.

In contradistinction to general paresis, the story of pellagra is one where the major phenomenological aspects of the disease were established early. However, the essential cause of pellagra was not discovered until 200 years after it was first observed, and some two decades after a method for controlling the disease had been determined. The latter point is, of course, not unusual. Diabetes mellitus is a good example of an extremely complex metabolic disease in which the accumulation of knowledge and the elucidation of one major link in the causal chain has led to a life-saving form of treatment even though the actual cause of the condition is unknown. How then can we understand the pattern of discovery in pellagra? To answer this question, a brief review of the etiologic theories current in the nineteenth century will be useful.

There were two basic explanations. One derived from the imputed affinity of pellagra to ergotism. Spoiled grain was known to cause ergotism, and by analogy it was suggested that pellagra was due to damaged maize. Pellagra was thus conceived as the consequence of a toxic process produced by a poison arising out of decomposition changes in maize. The function of the level of knowledge as a perceptual frame is clearly evident in this theory.

The second theory explained pellagra in terms of a protein deficiency. It had been observed that the disease occurred in epidemic form, particularly when there were extreme shortages of food among peasant populations. Yet it was also known that the disease appeared in people who were not among the poorest, but who lived very largely on maize. According to this theory, pellagra was due to the low nutritive value of a maize diet. It appeared where maize formed an exclusive or at least a preponderant part of the diet, and was due specifically to the small amount of nitrogen in it. In short, pellagra was a nitrogen, or protein deficiency disease.

These theories were based on biologic knowledge available in the nineteenth century. The bacteriologic discoveries at the end of that period tended to reinforce the view that disease was due to a pathogenic agent introduced into the organism. This biologically plausible explanation was not consistent with the idea that a disease such as pellagra could be caused by a deficiency of a metabolic constituent, especially a constituent of an unknown kind. And here lies the crucial significance of Goldberger's classic epidemiologic studies, in the irrefutable establishment of a biologically implausible situation, thus creating a whole new area for further study. At the same time, his investigations indicated the means for control of pellagra and its associated psychosis. But the precise biochemical factor, called PP (pellagra preventive) by Goldberger, was not identified with nicotinic acid until 1937.[38]

The history of cretinism is in some respects similar to that of pellagra.

Cretinism has been recognized as a clinical entity in Europe since the sixteenth century. The first published descriptions were those of Felix Platter (1602) and Paracelsus (1603). The former's account covers the salient features: disproportion of the body, deaf-mutism and mental retardation. Platter also observed that some cretins had goiters while others did not, and he noted the distribution of cretinism throughout the Alpine lands, particularly Switzerland and Carinthia. Since then good descriptions of cretinism have been given by numerous investigators, and it has been possible to classify this condition into five types: endemic cretinism, congenital thyroid aplasia, familial congenital goitrous cretinism, acquired athyroidism, and acquired hypothyroidism.[39] This classification based on current knowledge derives from the fact that absence or dysfunction of the thyroid gland are central to the production of the cretinous state. There is no doubt that in the eighteenth and nineteenth centuries idiots and other mental defectives were sometimes classed as cretins. However, this did not basically hinder the identification of cretinism and hypothyroidism by Gull in 1873.[40] Furthermore, thyroid medication was a logical sequence of the recognition of the unity of myxoedema and cretinism, and of the role of the thyroid in their etiology.

Nevertheless, the causation of endemic cretinism has been marked by controversy since the condition was first described. For example, the relationship of endemic goiter to endemic cretinism is still in dispute. More recently the role of genetic factors, especially as an aspect of intermarriage in relatively isolated cretinous districts, has been emphasized in this connection. Suggestive also is Clement's comment that "the discoveries in respect of inherited deafness and congenital deafness, the sequel of maternal rubella, have clearly revealed the need for making careful inquiries into the family history of persons with deaf mutism alone in goitrous areas before labeling them the victims of endemic goitre."[41] Clearly, the problem of cretinism in terms of pathogenesis is still far from solved, even though the condition has been well defined clinically, and much is known about thyroid function in relation to the severity of clinical phenomena.

What Does It Mean?

What inferences, if any, can be drawn from the examples discussed above. I believe they may be set forth as follows: There is a pattern of discovery in the sense that the problem must first be defined in clinical terms. This is more easily accomplished in certain cases; it is more difficult in others. Clinical conceptualization depends to a considerable degree on the acuteness of the morbid process and on the striking character of the presenting signs and symptoms. Pellagra and cretinism are illustrative on this point. Furthermore, diseases have a history not only in the sense that they appear at a given point in time, e.g., pellagra, but also in the sense that some diseases change their character over time, from acute to chronic, from virulent to mild. Account must be taken of such aspects in any endeavor to conceptualize diseases, especially mental illnesses which in so many instances are long-term in character.

Clinical conceptualization, however, is only one aspect of the pattern of discovery. The second is the relation of clinical data to other kinds of data—statistical, biochemical, sociologic, or psychologic. As has been pointed out, these types may be considered as levels, and the pattern of discovery for a given problem is established in terms of the relations between the various levels involved. If to this relationship is added the dimension of time, the pattern of discovery may be conceived as a process, or as a pattern of interactions out of

which under given conditions there can emerge a whole or partial answer to the initial disease problem.

What then are the implications for mental illness? In the first place, the history of mental illness may yield clues that are not otherwise apparent. For example, as cultures change in history, new styles of mental illness arise: and in the same culture psychopathologies differ at different periods. In Europe, the various outbreaks of the dancing frenzy in the thirteenth and fourteenth centuries gave way to epidemics of possession by demons from the fifteenth to the eighteenth centuries. These activities of the Devil were in turn displaced by the hysterical convulsions and twitchings of the eighteenth and nineteenth centuries, and in our day the psychosomatic disorders seem to have pride of place.

Second, there is available considerable knowledge relevant to mental illness or organ physiology, psychodynamics, and the functioning of small and large social systems. Nonetheless, still more knowledge is needed, for instance, on the functioning of the nervous system in biochemical and biophysical terms. Even more significant, however, would be endeavors to take knowledge already available in the various areas that have been mentioned, and to design studies which would explore patterns of relations among them. This is the direction in which history points. This is what happened in the past, and what will have to happen in the future if the mental diseases are to be understood sufficiently so as to be amenable to control.

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Designing for Underground Dwellers

Pennsylvania State University's College of Engineering and Architecture has initiated a Shelter Research and Study Program relating to the planning, design. and analysis of shelters and structures to resist the effects of nuclear weapons.

In the government-sponsored research program investigators will look into "the architectural engineering aspects of the shelter program—as well as the psychological. physiological. sociological, and economic aspects." A comprehensive, integrated approach to studying the various requirements for habitability in shelters is required if a national underground protection and housing program is to be logically and economically planned. The college, noting that such an approach has long been wanting on the national scene, is now taking steps to fill the need through both research and curricula.

Merritt A. Williamson, P.E., is dean. The program is under the direction of Professor Gifford H. Albright, assisted by Professor Allen F. Dill. In addition to other college staff members, faculty will include specialists in psychology, sociology, and physiology and in other disciplines of the university.